If You Build It, Will They Come?
School Availability and School Enrollment in 21 Poor Countries

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Abstract

Increasing the supply of schools is commonly advocated as a policy intervention to promote schooling. Analysis of the relationship between the school enrollment of 6 to 14 year olds and the distance to primary and secondary schools in 21 rural areas in low-income countries (including some of the poorest countries in Sub-Saharan Africa) reveals that the two are often statistically significantly related. However, the magnitudes of the associations are small. Simulating big reductions in distance yields only small increases in average school participation, and only small reductions in within-country inequality. The data are mostly cross-sectional and therefore it is difficult to assess the degree to which results might be driven by endogenous school placement. Data can be geographically matched over time in three of the study countries and under some assumptions the results from these countries are consistent with no substantial bias in the cross-sectional estimates. Although increasing school availability by decreasing the average distance to schools can be a tool for increasing enrollments, it cannot be expected to have a substantial effect. Other interventions, such as those geared toward increasing the demand for schooling or increasing the quality of schooling should be prioritized.

1. Introduction

With more than 100 million children of primary school age not in school around the world—the majority of those in the poorest countries of Africa and Asia—the task of achieving Education for All is a difficult one. A range of policy interventions to increase school participation are typically discussed: for example increasing the availability of schools; providing demand-side subsidies such as cash transfers that are conditioned on school participation; or promoting school quality as a way of increasing the value of schooling and therefore its perceived benefits (for an overview see World Bank 2004). In many instances, increasing the number of schools—thereby lowering the distance to schools for any given child—is an attractive option to policymakers as well as the international agencies who support them through funding. The intuitive appeal of school construction as a pre-requisite to schooling is obvious. In addition, building schools is a fairly straightforward task, especially when compared with the more difficult job of ensuring high quality learning in a vast number of schools around a country. Moreover, the political benefits of sponsoring the construction of a visible and tangible school are potentially greater than those accruing to supporting efforts to ensure quality—an elusive concept at best.

But how much should one expect school enrollment to increase as a result of such a "bricks-and-mortar" approach to expanding education opportunities? This paper investigates the answer to this question by simulating the impact of lowering the average distance between a child and schools on school enrollment using data from 21 low-income countries. The results imply that although increasing school availability can be a tool for increasing enrollments, it cannot typically be expected to have a large effect. Estimates of the association between the distance to the nearest school, or the availability of a school in a close geographic area, are often statistically significant—but they are typically very small in magnitude. For example, in rural Chad where only 26 percent of 6 to 14 year olds are reported to be in school, reducing the distance to all primary schools to zero would be associated with only about a 6 percentage point increase in school participation. In only a small number of countries—particularly those in which the average distance is high—would reducing the distance to the nearest secondary school be associated with a sizeable increase in the enrollment of 6 to 14 year olds (who are mostly of primary school age). Moreover, there is little evidence that increasing school availability is associated with large changes in inequality—by household wealth or by gender—in school participation. However, in some countries, particularly where overall enrollment is low, increasing school availability may initially exacerbate inequalities in enrollment.

The data used are mostly cross-sectional and therefore it is difficult to assess the degree to which results might be driven by endogenous school placement: if schools are placed in communities where they are "most needed" (in the sense that in their absence enrollments would be lowest) one could observe a small cross-sectional association that could still be consistent with a large causal impact of increasing school availability on enrollment. Results for the three countries for which data can be matched geographically over time, however, are not consistent with substantial bias in the cross-sectional estimates (on the assumption that school placement is determined by geographically-fixed characteristics).

The results imply a limited potential impact of increasing school availability on school enrollment. Other interventions, such as those geared toward increasing the demand for schooling or increasing the quality of schooling, should be prioritized—or at least be substantive complements to a school construction effort.

2. The responsiveness of schooling to cost and school availability

Why would one expect a response?

As the price of a good decreases, demand for it will increase if other things remain equal. School fees are only one part of the price of schooling—the explicit part of the cost of schooling that is borne by parents and children. One of the main costs of schooling is the travel time to school. This time can be used neither for productive activities nor for leisure. It is time spent solely for the benefit of acquiring education, but it is not spent on the task of learning. In addition to these economic valuations of the cost, there is the personal discomfort of long distance travel—frequently on foot in poor countries. One would therefore expect that reduced distance and travel time should be associated with higher enrollment.

The relationship between distance to school and the probability of enrollment may differ for different children. The value of travel time will depend on the individual child's opportunity cost of time, which may differ by the gender of the child. For example, if primary school age girls look after their siblings, freeing up their mothers to engage in higher productivity activities, then time lost could be valued quite highly. Boys might participate in income generating activities, for example farm work, that contribute directly to household income. Again this will determine the value of the time lost traveling to school. The non-economic aspects of travel time may differ by gender of the child as well. For example, traveling long distances on foot might pose more of a personal safety issue for young girls than young boys, which would lead one to expect that the enrollment of girls would be more sensitive to travel time than that of boys. Similarly, the value of lost time might differ systematically for richer or poorer children. Children in poorer families might contribute relatively more to household income, and therefore the time they spend traveling to school might be relatively more valuable.

Clearly, reducing the time it takes for children to reach school should increase enrollment. Whether it contributes to reducing inequalities in enrollment is less clear. But distance to school is only one of many factors that influence the decision to enroll. Any model of the demand for education will depend on the family's myriad perceived costs of and benefits to schooling. The question therefore remains: how much should one expect average enrollment to increase if travel time could be decreased, and to what extent might such an increase help to reduce inequalities in enrollment?

Some recent findings on the relationship between distance to school and enrollment

The literature on the relationship between access to school and enrollment is difficult to summarize. Often the distance to school is included in an analysis as a control variable but is not the focus of attention and therefore its importance (or statistical significance) is not discussed.[1] Perhaps more pernicious, it is easy to imagine that studies that set out to analyze the relationship between distance to school and enrollment and find no effect do not get wide distribution (or are not written at all if preliminary data analysis finds no association). Moreover, even among studies that have focused on the question and have been published, methodological differences make it difficult to draw strict comparisons. Some relatively recent results do stand out, however. In particular, four studies explicitly attempt to account for the potential endogeneity of school availability and school enrollment:[2]

- A panel data study of over 4,000 households in rural India between 1971 and 1982 found that the new construction of a school in a village significantly increased the probability that a child aged 5 to 14 was enrolled (Foster and Rosenzweig 1996). The point estimates imply that building a school in a village can more than double the enrollment rate—although only about 6 percent of villages did not have a school at the start of the study period.
- A retrospective study of a large-scale school construction program in Indonesia between 1973 and 1980 found that additional schools led to higher average education attainment. The program, which resulted in increasing the number of schools by more than 1 per 500 children resulted in on the order of .2 to .4 additional years of schooling for those children (Duflo 2001). The impact of the program was larger in poor regions.
- Using a different methodology, a panel study between 1976 and 1986 showed that the change in the density of schools significantly affected school attendance of children ages 10 to 14 and somewhat less significantly those ages 15 to 18 (Pitt, Rosenzweig, and Gibbons, 1993).
- A repeated cross-section study of a large-scale program that emphasized school construction in India between 1993 and 1999 found that the enrollment of children ages 6 to 10 only increased by about 1 percentage point, and that of children 11 to 13 actually decreased (Jalan and Glinskaya 2003).

Other studies use cross-sectional analysis to address the issue—but cannot deal with potential endogeneity:

- In rural Ghana, a bigger distance to all types of schools—primary, middle, and secondary—reduced the probability that a child aged 5 to 12 had ever been to school, and reduced the number of years a child would spend in school (Lavy 1993).
- In rural communities in Northwest Tanzania, distance to secondary school mattered for girls' enrollment in primary school—but not boys'—ages 10 to 15 while the presence of more primary schools in a village affected neither (Beegle and Burke 2003).

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[1] For example Alderman, Behrman, Lavy and Menon (2001) focus on the impact of health on enrollment, but their model includes distance to school which is a statistically insignificant correlate of the decision to enroll in school.
[2] See the discussion below for more on this issue.

- In Tanzania, overall, the distance to the nearest primary school increased the age at which a child enrolled in school, and shortened the number of years spent in school (Bommier and Lambert 2000).
- Longer travel time to the nearest school in Mozambique was significantly negatively related to enrollment, with boys being more sensitive to distance than girls (Handa 2002).
- Travel time to the school was not associated with primary level enrollment in Peru (Younger 2000). Enrollment at the secondary level was responsive to distance to school: on average, a one-kilometer increase in the distance was associated with about an 8 percent decline in enrollment (Younger 2003). The latter study interacts distance to school with household income and finds that the interaction is not statistically significantly different from zero, suggesting that the effect is neither larger nor smaller for poor children.

Despite the variability in methodologies of these published results, it is clear that there is enough to support the notion that distance can matter for enrollment. There is a wide range in terms of the estimated magnitudes of the impact—and ambiguity on whether it matters more for girls. Of the two studies that interact availability and poverty, one suggests that the poor are more responsive to availability (Duflo 2001) the other that they are not (Younger 2003).

3. Using demographic and health survey data for analyzing school participation and distance to schools

DHS data

This paper uses 24 data sets from 21 countries to consistently investigate the relationship between enrollment and school availability. The data are from Demographic and Health Surveys (DHS) carried out in the 1990s. This is a particularly attractive collection of data sets. They are large-scale household surveys that aimed to be nationally representative—the sample of children aged 6 to 14 was never less than 2,500 (Cameroon) and exceeded 5,000 in all but 7 of the 24 data sets (Table 1). The data collection methodologies (sampling strategies and survey instruments) were very similar across all the surveys. In the countries analyzed here, unlike most countries with a DHS, the survey included a community level module covering at least rural areas that asked about the distance to various facilities and amenities, including primary and secondary schools.

The percentage of children aged 6 to 14 enrolled in school ranges substantially across these data sets: from 10.4 in Niger (1992) to 83.8 percent in Bolivia (Table 1). Likewise the availability of primary schools—as measured by the distance to the nearest school—ranges from 0.2 kilometers in Bangladesh to over 7 kilometers in Chad with an unweighted average across these data sets of about 2 kilometers. Access to secondary schools is much more restricted. The average distance to the nearest secondary school ranges from 1.8 kilometers in Bangladesh to over 71 kilometers in Mali, with an unweighted average of 21.5 kilometers. Across countries there is a sizeable correlation between enrollment and the average distance to schools. The correlation coefficient is –0.64 between the enrollment of 6 to 14 year olds and the average distance to the nearest primary school, and –0.76 between their enrollment and the average distance to the nearest secondary school.

Table 1: Average marginal effects of school distance from model of school participation and summary statistics, rural children aged 6 to 14 (probit model)

	Summary statistics						Average marginal effects of distance		
	School participation	Distance to nearest primary school		Distance to nearest secondary school			Distance to nearest primary school (km)	Distance to nearest secondary school (km)	P-value: Joint test
	Mean	Mean	Std. Error.	Mean	Std. Error.	N			
Niger 1992	0.104	3.5	(6.3)	36.0	(31.5)	5171	-0.009 *	0.000	0.111
Niger 1998	0.146	2.4	(8.4)	29.3	(21.9)	6582	-0.031 **	-0.001 **	0.000
Mali 1995-96	0.161	6.6	(8.7)	71.3	(29.6)	8913	-0.016 **	0.000	0.000
Senegal 1992-93	0.182	5.0	(10.0)	37.4	(31.3)	4150	0.004 **	0.000	0.022
Burkina Faso 1992-93	0.195	2.4	(6.0)	25.2	(21.1)	5848	-0.024 **	0.000	0.000
Chad 1998	0.264	7.5	(13.1)	38.3	(40.5)	5636	-0.017 **	-0.002 **	0.000
Benin 1996	0.337	0.9	(3.6)	24.2	(28.6)	5204	-0.019 **	-0.004 **	0.000
Morocco 1992	0.374	2.1	(4.3)	28.6	(25.7)	4259	-0.002	0.000	0.990
Côte d’Ivoire 1994	0.417	1.1	(5.8)	34.9	(29.5)	5638	0.000	-0.001	0.430
Tanzania 1991-92	0.454	1.0	(5.9)	31.8	(28.8)	9899	-0.007	0.000	0.186
C.A.R. 1994-95	0.465	4.0	(9.4)	44.2	(29.8)	3923	-0.012 **	0.001	0.000
Madagascar 1992	0.521	0.9	(2.6)	11.6	(14.2)	5569	-0.030 **	0.000	0.000
Dominican Rep. 1991	0.530	0.5	(1.2)	8.7	(7.7)	3343	-0.024 *	0.000	0.118
Nigeria 1999	0.611	1.0	(3.2)	6.0	(12.6)	6048	-0.003	-0.003	0.073
Cameroon 1991	0.625	2.5	(6.2)	24.4	(24.7)	2531	-0.009	-0.006 **	0.001
India 1992-93	0.629	0.2	(2.3)	5.2	(10.5)	73023	-0.001	-0.001	0.287
Haiti 1994-95	0.676	1.5	(3.7)	10.0	(14.1)	3507	-0.015 **	0.000	0.023
Uganda 1995	0.679	1.4	(1.8)	7.0	(6.9)	6803	-0.006	-0.004	0.020
Bangladesh 1993-94	0.704	0.2	(0.6)	1.8	(2.0)	10736	-0.021 *	-0.013 **	0.001
Bangladesh 1996-97	0.732	0.2	(0.6)	2.0	(4.2)	9774	-0.028 *	-0.004	0.113
Philippines 1993	0.752	0.5	(4.2)	8.2	(21.3)	8896	-0.001	0.000	0.529
India 1998-99	0.759	0.4	(3.7)	4.5	(6.8)	76147	0.000	-0.001	0.285
Zimbabwe 1994	0.836	2.5	(4.6)	7.2	(6.6)	6741	-0.005 **	-0.003 **	0.000
Bolivia 1993-94	0.838	0.7	(1.7)	18.1	(25.3)	4184	-0.008	-0.001 **	0.003

Note: Data ordered by average school participation. Selected results from a model that includes child’s age, adults’ education, characteristics of the head of household, and a set of cluster characteristics.*, ** indicate significance of the underlying groups of probit coefficients at the 5, and 1 percent, respectively.
Source: Author’s calculation from DHS data.

The DHS have disadvantages as well. The surveys were not designed for the specific purpose of carrying out analysis of education. Therefore, the timing of the surveys was not synchronized with the academic year, leading to potential confusion over the meaning of “in school,” the phrasing of the question relating to school enrollment.

Another disadvantage is that the surveys do not collect information on household expenditures, the variable typically used in poverty analysis. This analysis uses an alternative to household expenditures to investigate the relationship between enrollment and household long-run income. The DHS collect information on the ownership of various household assets, as well as indicators of the quality of the dwelling in which the family lives. These can be aggregated into an index that performs well when compared to expenditures (i.e. gives similar results) and predicts enrollments even better in some data sets (Filmer and Pritchett, 2001). Individuals are ranked by the index of the households they live in and are assigned to the "richest" and "poorest" 50 percent. The approach is the similar as in Filmer (2000) where the focus is on gender and wealth differences in enrollments in a larger number of DHS data sets.

Distance to school is typically recorded at the cluster level by a community respondent. That is, an informed villager (such as a teacher) or a local government official might be asked about the availability of services in the sample cluster. Since the data analysis is carried out at the individual level, the fact that the distance to school is a cluster aggregate introduces some degree of measurement error. That is, some respondents might be closer to the school than the reported distance, some further. Short of asking each household respondent the distance to the nearest school (which is also likely to be reported with error) or GPS location identification and travel-time mapping (which had not been generally instituted at the time of the data sets collected here) it is difficult to get around this problem. One way is to use a measure of whether there is a school in the cluster, a less precise indicator of school availability but one that is much less susceptible to measurement error. As discussed below such an approach yields qualitatively similar results.

The role of school quality

A key aspect of using DHS data for this analysis is that the data contain no information about the nearest schools other than their distance. In particular there is no information about the quality of the schools that children have access to. While there is a relatively substantial "production function" literature that relates school characteristics—including quality indicators—to test scores (see for example a review in Pritchett and Filmer, 1999), there has been limited analysis of the association between quality and enrollment or grade attainment (Lloyd and others, 2001). The limited results do suggest that school quality matters for school attainment:

- Lavy (1996) found that some schooling quality indicators were associated with children ever having enrolled in school, as well as the number of grades they ultimately complete in Ghana.
- Bommier and Lambert (2000) found that, in Tanzania, the perceived quality of mathematics instruction was associated with younger enrollment age, and that the perceived quality of Swahili instruction was associated with higher grade attainment.
- Younger (2000, 2003) found that indicators of the quality of the school's physical environment, as well as of the staff, were statistically significantly associated with enrollment at the primary and secondary levels in Peru.
- Paxson and Schady (2002) found that an increase in expenditures devoted to improving the physical quality of schools in Peru was associated with increases in school attendance between 1993 and 1996.
- Lloyd and others (2003) found that school quality indicators (such as the school's physical environment, the qualifications of teachers, and some measures of the behavior of teachers) were significantly associated with the grade attainment of adolescent girls in

Egypt. A parallel study in Kenya found that school environment factors (in particular the differential treatment of girls relative to boys) contributed to differential dropout among girls and boys (Lloyd and others 2000).

Since the DHS data do not have any information on the quality of the nearest school, they cannot be used to for this type of analysis. The associations and simulations described in this paper can therefore only be interpreted conditional on the average quality actually observed in the data. For example, to anticipate the results for some countries: if an increase school availability can only be expected to raise enrollments by a small amount this should be interpreted as the impact of an increase in the availability of schools with the same average quality as existing schools.

4. An empirical model assessing the relationship between enrollment and availability

The methodological approach used here is to estimate a multivariate model relating school enrollment to child, household, and community variables—including distance to the nearest schools. For the purposes of the analysis, community is defined as the sampling "cluster" used in the survey. A cluster is the primary sampling unit in the DHS surveys. There are typically between 20 and 30 households interviewed per cluster. Community questionnaires were administered at the level of these clusters. Since these questionnaires were limited to rural areas in many countries, and since distance to schools is likely to be more of a salient issue in rural areas, the analysis is limited to households living in rural areas.

The specific variables included in the model are:

- *Child level*: gender (dummy for male), age, age squared
- *Household level*: household wealth (dummy variable for being in the top 50 percent of an index derived from household asset ownership and housing characteristics), the highest years of schooling of adult males in the household, the highest years of schooling of adult females in the household, the gender of the head of the household (dummy for male), the years of schooling of the head of the household
- *Cluster level*: distance to nearest primary school, distance to nearest secondary school, distance or availability of various cluster level facilities.

The cluster level variables include more than just the distance to a primary school, the school level that 6 to 14 year old children are likely to be attending. Although these children are not likely to be attending secondary school, the access to secondary places may have an impact on primary schooling and the dummy variables for secondary school are therefore included in the multivariate analysis (consistent with Lavy 1996).

The equation includes a set of community facility and infrastructure variables in order to ensure that a relationship with school presence is not simply reflecting the fact that communities with more facilities and infrastructure in general, including schools, may tend to have higher enrollment (consistent with Pitt, Rosenzweig and Gibbons 1993). While the exact list of variables varies from survey to survey, the typical list includes: the distance to the nearest urban center; the distance to the nearest post office, local market, bank, cinema, public transport, pharmacy, health center, hospital, or clinic.

5. Results and discussion

Is average enrollment associated with school availability?

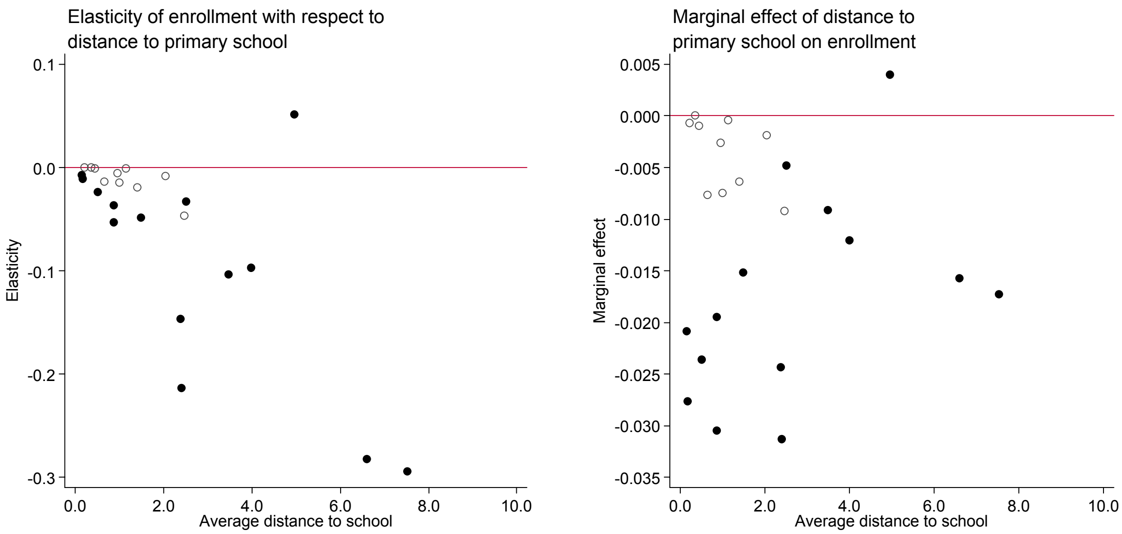
The last three columns of Table 1 report selected results from estimating the school enrollment model on 24 data sets in the 21 countries (variables other than distance are not reported—these are available from the author). The model is estimated as a Probit model with distances specified linearly (see below for a discussion of different specifications). The distance to the nearest primary school is statistically significantly negatively related to enrollment in all but nine of the data sets (at the 5 percent level). The distance to the nearest secondary school is positively associated with enrollment of 6 to 14 year olds in 7 of the 24 of the data sets.

The left panel of Figure 1 explores patterns in the results by showing, on the vertical axis, the elasticity of enrollment with respect to the distance to the nearest primary school, and on the horizontal axis the mean distance to the nearest primary school in each data set (solid points indicate those where the underlying coefficient on distance to the nearest primary school is significantly different from zero).[3]

There is a strong negative association: as mean distance goes up the elasticity grows more negative (the correlation across the 24 data sets is 0.73). A large part of the association, however, comes from the fact that mean distance enters into the denominator of the calculation of the elasticity: the correlation between the marginal effect of distance on enrollment and mean distance is much weaker (0.002) and the association is illustrated in the right panel of Figure 1. Nevertheless, a general pattern does stands out: when the mean distance to the nearest primary school is large there tends to be a reasonably sized impact of distance on enrollment, but when mean distance is relatively low the impact varies a lot—some countries have a large impact and some have none.

[3] The elasticity is defined as the average marginal effect of distance on enrollment, times mean distance, divided by mean enrollment. Its interpretation is that of the percent change in enrollment corresponding to a one percent change in distance.

Figure 1: Relationship between the elasticity of enrollment with respect to the distance to the nearest primary school and mean distance to the nearest primary school



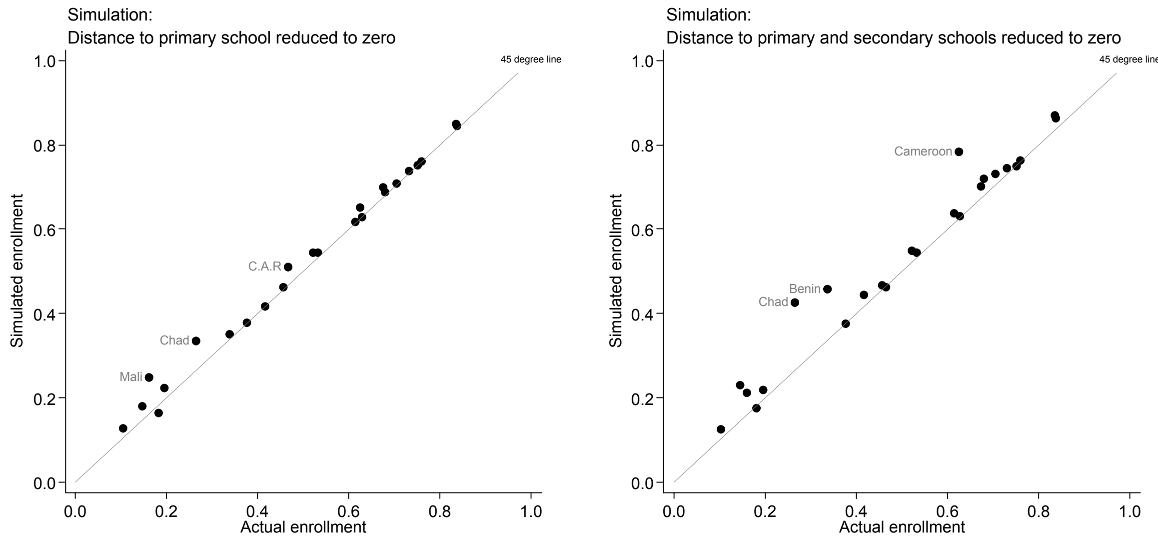
Source: Author's calculations from DHS data.

How should one interpret these results? With distance being statistically significantly related to enrollment, can one infer that distance is "important"? Figure 2 illustrates a simulated scenario based on the parameter estimates in Table 1. The horizontal axis is the actual enrollment rate, the vertical axis is the simulated enrollment rate if the distance to all schools was set to zero (that is, corresponding to a school in every cluster). The left panel shows the result of setting only the distance to the nearest primary school to zero, the right panel shows the result of setting distances to both the nearest primary and secondary school to zero.[4] The graph illustrates the maximum impact on enrollment that one might expect from a policy that focused exclusively on putting a school in every village.

Clearly, distance to the nearest school is only weakly associated with enrollment: most points lie very close to the 45-degree line. There are only three countries in which a primary school in every cluster is associated with more than a 4 percentage point increase in enrollment: Mali, Chad and Central African Republic (8.6, 6.8 and 4.4 percentage points respectively). This is a small increment given both the overall shortfall from universal enrollment in these countries, as well as the number of new schools that would be implied by this simulation. In Mali, only about 35 percent of the surveyed clusters had a primary schools—this means that over doubling the number of clusters with a school would only be associated with an enrollment increase of 8.6 percentage points.

[4] The simulation involves comparing the average predicted enrollment with each observation at actual values to the average predicted enrollment with each observation at actual values except for distance which is set to zero.

Figure 2: Simulated enrollment impact of reducing distance to schools



Note: The vertical distance from the 45^o line shows the change in proportion enrolled associated with reducing the distance to school to zero. Labeled countries are those for which the absolute value of the change is greater than .04 for the left panel, and 0.1 for the right panel.
Source: Author's calculations from DHS data.

More countries stand out with a substantive enrollment increase in the simulation of reducing the distance to both primary and secondary schools to zero. Cameroon, Chad and Benin stand out with an implied increment of more that 10 percentage points. The largest increment to enrollment is in Chad where the simulation implies a 16 percentage point increase in enrollment associated with a reduction of the distance of all primary and secondary schools to zero. Only 4 percent of children in Chad live in clusters with a secondary school so the 16 percentage point increment would come at the cost of a massive investment in school construction.

Endogenous school placement

Endogenous program placement is a problem that plagues this type of study. The problem arises if schools are placed where enrollment would be lowest in their absence. If this were true, and the availability of schools actually increased enrollment, then one could observe no difference according to distance but this would be an artifact of the selective nature of school placement, that is a bias towards finding a small association between distance and enrollment. However, alternative scenarios could lead to a bias in the opposite direction. For example, families who value schooling a great deal might move to live closer to schools. One could therefore observe a big fall in enrollment as distance increased, but this would be overstating the impact of availability. Similarly, policymakers might locate schools in locations where populations are more vocal, perhaps because they are more politically powerful or because they value education more. Again, in this case the cross-sectional analysis would overstate the impact of availability on enrollment.

Studies have used different methods to adjust the estimated effect. Pitt, Rosenzweig, and Gibbons (1993) use fixed effects to control for fixed location attributes that might drive the selective placement process. Foster and Rosenzweig (1996) add instrumental variables to a household fixed-effects approach to allow for the fact that time-varying effects might be important. Duflo (2001) uses a quasi-experimental approach that exploits the timing of school placement, and variation across regions in its intensity, to identify the impact. Jalan and Glinskaya (2003) use a combination of comparing changes in enrollment over time with matching individuals in districts who received an education program with those in districts that did not.

Pitt, Rosenweig, and Gibbons (1993)—studying of the supply of schools and school enrollment in Indonesia between 1976 and 1986—find that the relationship between the enrollment rate of children 10 to 14 and the density of primary schools roughly doubles when moving from a cross-sectional estimate to one that includes fixed effects. The estimate of the relationship with secondary schools remains about the same. The point estimates are small however. For example, cross-sectional estimates indicate that an increase in the percentage of households living in villages with primary schools from 74 (coverage in 1980, the first year of their panel data set) to 100 percent would be associated with an increase in enrollment by about 1.1 percentage points, whereas the fixed-effects estimates imply that enrollment would increase by about 2.5 percentage points. The authors conclude that the growth in the spatial coverage of schools played a relatively small (albeit statistically significant) part in the observed increase from 79 to 89 percent in school enrollment of 10 to 14 year olds between 1980 and 1986. On the other hand, the fixed-effects methodology suggests that the enrollment of 15 to 19 year olds was not responsive to secondary schools coverage over this period (during which enrollments increased from 34 to 49 percent).

Foster and Rosenzweig (1996) study agricultural productivity and education changes in rural India between 1971 and 1982. They use instrumental variables based on estimated local productivity shocks, in addition to village fixed-effects, to allow for the fact that high economic growth might result in increased demand for education in some areas, to which governments might respond. They do not report cross-sectional estimates so the overall effect of their methodology cannot be inferred. They do, however, report the comparison of the fixed-effects to the fixed-effects with instrumental variables (which are estimated as a residual from a model of productivity). Simple fixed-effects yields estimates that the new construction of a school is not statistically significantly associated with the enrollment of children aged 5 to 14, whereas adding the instrumental variables results in a highly significant and larger estimate of the impact of school construction. The results imply that building a school in a village can more than double the enrollment rate—although only about 6 percent of villages did not have a school at the start of the study period so the identification of the impact is off of these, perhaps extreme, villages.

Duflo (2001) uses cross-sectional data but variation in the historical timing of school construction across regions to show the impact of school construction in Indonesia between 1973 and 1980—when the government made a major effort at building primary schools. The program built about one new school for every 500 children of primary school age. The analysis exploits the fact that the program represented a significant change in policy and therefore older cohorts were not exposed to it, and that it was implemented at different rates in different regions, in order to identify the causal impact. Based on the results presented it is not possible to assess what the results of a cross-sectional analysis would have been. Duflo (2001) estimates that the program resulted in 0.27 additional years of schooling for each child (from a starting point of an average of almost 7.98 years—or an increase of 3.4 percent in years of schooling) with a larger effect in poorer areas.

Jalan and Glinskaya (2003) use difference-in-difference propensity score matching to compare school enrollment in districts of India that received a large-scale education program (that included a large school construction component) with those that did not receive the program. Their data from 1993/1994 and 1999/2000 bracket the intervention which was launched in 1994 and which targeted districts with high female illiteracy. They find that simple estimators that compare enrollment before and after the program *overstate* the impact of the program on enrollment. For example, enrollment of 6 to 10 year olds in program districts increased by 5.4 percentage points, but this was only 2.0 percentage points above the increase in non-program districts. Moreover, it was only 1.3 percentage points higher than the increase in comparable non-program districts (as selected by their propensity score matching methodology). In addition, the enrollment of 11 to 13 year olds in program districts increased 3.3 percentage points, but this was actually 2.6 percentage points less than those in comparable non-program districts. Their main finding is the overall limited impact on enrollment of what was a large and expensive intervention.

The cross-sectional nature of DHS data (and limited complementary information such as the timing of school construction) makes it difficult to systematically deal with the potential endogeneity of schools. In three of the countries analyzed here there have been multiple surveys that can be matched, to some degree or another, across time according to geographic location.[5] A fixed effects approach can therefore be used in these cases. Interpreting the results as the causal impact of the availability of schools on enrollment rests on the assumption that any cause of the selective placement of schools is geographically fixed over time. As such, this approach is most similar to Pitt, Rosenzweig and Gibbons (1993).

Table 2 reports selected results from models that estimate the association between distance to schools and enrollment:

- in each of the years for which data are available;
- for a sample that pools data across the two years;
- and for the pooled sample using models that include fixed effects for geographic location.

[5] Many countries have multiple DHS surveys but do not have the requisite cluster level variables on the availability of schools and other services.

The table reports only the marginal effect estimates of distance to primary and secondary schools on enrollment. The models include all the covariates included in the models underlying Table 1 that were collected in both years. Unlike the models in Table 1 the variable that controls for household wealth is constructed from data pooled across the two years.

In Niger, data can only be matched at the regional level so the model for that country includes fixed-effects for the six regions. In India the data can be matched at the district level so two models are estimated: one with fixed-effects for the 26 states, and one with fixed-effects for the 440 districts. In Bangladesh the data can be matched all the way down to the cluster level so models that include region, district, or cluster fixed-effects can be estimated.

The results from Bangladesh suggest that controlling for the fixed cluster attributes may well affect the magnitude and significance of the results—but not always in a way that biases against finding large impacts of distance on enrollment. The cross-sectional results suggest that distance to primary school is significantly associated with enrollment (at least at the 10 percent level). But the cluster fixed-effects estimates imply that the impact of distance is smaller in absolute value (–.011 versus –.023 in 1993-94 and –0.029 in 1996-97) and no longer significantly different from zero. In this case the cross-sectional estimates *over*-estimate the impact of distance on enrollment. Introducing fixed-effects at intermediate levels (state, district) results in progressively smaller point estimates.

Table 2: Marginal effects of school distance variables from probit model of school participation, rural children aged 6 to 14

	Marginal effects			
	Distance to nearest primary school		Distance to nearest secondary school	
	Marginal effect	Standard error	Marginal effect	Standard error
Bangladesh				
1993-94	-.0225	.0103*	-.0136	.0041**
1996-97	-.0291	.0147	-.0042	.0066
Pooled	-.0264	.0096**	-.0084	.0039*
Pooled region fixed-effects (5 groups)	-.0232	.0090*	-.0081	.0040*
Pooled district fixed-effects (61 groups)	-.0179	.0058**	-.0103	.0022**
Pooled cluster fixed-effects (258 groups)	-.0110	.0106	-.0123	.0040**
India				
India 1992-93	-.0008	.0015	-.0007	.0006
India 1998-99	.00002	.0007	-.0009	.0006
Pooled	-.0004	.0006	-.0008	.0004
Pooled state fixed-effects (26 groups)	-.0004	0.0008	-.0010	.0004**
Pooled district fixed-effects (440 groups)	-.0004	.0008	-.0012	.0003**
Niger				
1992	-.0077	.0033*	.00002	.0002
1998	-.0237	.0041**	-.0010	.0003**
Pooled	-.0123	.0034**	-.0004	.0003
Pooled region fixed-effects (6 groups)	-.0124	.0034**	-.0004	.0003

Note: Selected results from a model that includes child's age, adults' education, characteristics of the head of household, and a set of cluster characteristics. Cluster characteristics are selected to be consistent across the two rounds of data and therefore have minor differences with those in the models included in Table 1. *, ** indicate significance of the underlying probit coefficients at the 5, and 1 percent, respectively.
Source: Author's calculation from DHS data.

In India, the results are more ambiguous. The cross-sectional results for the distance to the nearest primary school are insignificantly different from zero, as are the fixed-effects estimates. District fixed-effects yields a marginal effect estimate that lies between the cross-sectional year-specific ones. The marginal effect of the distance to the nearest secondary school is insignificantly different from zero in the cross-sectional and state fixed-effects estimates, but becomes larger in absolute value and statistically significant in the district fixed effects estimates. In Niger, region fixed-effects yield estimates that are between the cross-sectional year-specific ones. It is possible that this could just mean that regional fixed-effects are not sufficient to take account of the relevant local attributes and therefore the results just give an "average" of the cross-sectional results.

These exploratory results from three countries do not suggest that cross-sectional estimates systematically understate the impact of distance on enrollment—which would bias the results toward the small impacts described above. Of course the exploration has limitations: in only two cases can the fixed-effects be estimated at a level that one might reasonably expect school placement decisions to be made at (districts in India, districts and clusters in Bangladesh). They rest on the assumption that attributes that are geographically fixed would drive any selective placement. Moreover, they are limited to only three countries where the requisite data are available and the problem of compensatory selective placement could differ across governments, for example, if more effective governments might be better at locating schools where enrollments would have been lowest in their absence.

To some, the inability to systematically control for potential endogeneity of the distance to school in DHS data might raise the question of whether it is worthwhile carrying out this multi-country analysis at all. The fact that the fixed effects in Bangladesh and India revealed no substantive bias is one finding supportive of going ahead. In addition, there are advantages to going ahead with implementing the same empirical specification across a number of data sets from a variety of circumstances. First, the approach avoids the issue of publication bias where non-significant findings are ignored. Second, it puts the variability of the estimated effects in focus—the elasticity of the distance to primary school ranges from -0.3 percent to 0 (or even positive in the case of Senegal) and a selective review of the literature emphasizing the largest findings could potentially overstate the "typical" response substantially. The results can certainly be interpreted as a summary of the conditional associations between enrollment and distance found in these data sets. Making the next step to interpret these as estimates of the causal effects is clearly less obvious, but the evidence suggests that it is not unreasonable.

Non-linearity in the relationship

The estimates so far have specified enrollment as a simple linear function of distance but the impact of distance on enrollment might vary.[6] Two alternative specifications of distance are explored: (1) a non-linear specification that estimates a probit model with distances included up

[6] Except to the extent that the probit model itself introduces an element of non-linearity.

to the 3rd power and (2) a specification that includes dummy variables for whether a primary school and/or a secondary school is in the cluster. The latter variables have the additional advantage of being measured with less error—albeit at the cost of being less precise estimates of school availability.

Table 3 reports the marginal effects of school distance and school presence derived from these two models, as well as the significance of the underlying probit coefficients.[7] The first columns show the average marginal effect of distance to the nearest school on enrollment based on the model where distances are specified up to the 3rd power and the middle columns show the average marginal effect of having a school in one's cluster, calculated as the average change in probability of enrollment resulting from a change in the dummy variable from zero to one (summary statistics on the percentage of children living in clusters with a school are reported in the last two columns).

The patterns of significance are generally consistent with those in the model with a linear specification of distance—although in some cases the point estimates when allowing for non-linearity in distance are quite different suggesting that flexible functional forms should be used for such an analysis.[8] Somewhat more countries are found to have a significant association between enrollment and school availability.

The model with the availability of schools in the cluster appears to suggest larger effects. For example, several of the models where the presence of a primary school in the village is significant imply that the effect is above 10 percentage points, and reaches over 20 percentage points in one case (Niger 1998). But these coefficients can be somewhat deceiving. The left panel of Figure 3 plots simulated enrollment with a primary school in placed in every village against the current enrollment. Again, relative to the policy goal of full enrollment, putting a school in every cluster will only get one so far. The only countries with more than a 5 percentage point increment to enrollment are Chad , Central African Republic, Mali, and Niger—where the biggest difference between actual and simulated enrollment is 12 percentage points in Mali.

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[7] In the case of the school presence model, it is the average marginal effect of taking clusters from no school to having a school on the probability of enrollment.

[8] For example, in Niger in 1992 the average marginal effect of distance to primary school is estimated to be about twice as large in the non-linear specification.

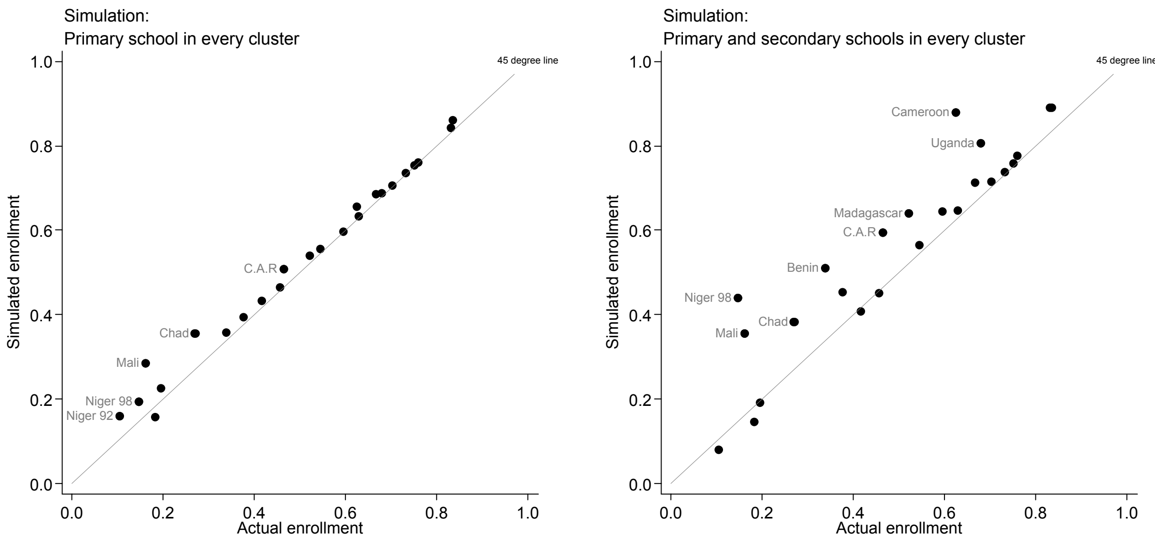
Table 3: Marginal effects of school presence or distance based on alternative specifications of distance, probit model of school participation, rural children aged 6 to 14.

	Alternative model 1			Alternative model 2			Summary statistics	
	Average marginal effect of distance to school based on non-linear function of distances			Average marginal effects of the presence of a school in the cluster (a)			Proportion of children living in clusters with:	
	Primary school	Secondary school	Joint test: p-value	Primary school	Secondary school	Joint test: p-value	Primary school	Secondary school
Niger 1992	-0.028 **	0.001	0.000	0.193 **	-0.084 **	0.000	0.528	0.113
Niger 1998	-0.058 **	-0.002 **	0.000	0.234 **	0.121 **	0.000	0.665	0.044
Mali 1995-96	-0.024 **	0.002	0.000	0.172 **	0.037	0.000	0.347	0.042
Senegal 1992-93	0.009 *	0.000	0.093	-0.065	-0.013	0.225	0.549	0.100
Burkina Faso 1992-93	-0.033 **	0.000 *	0.000	0.169 **	-0.030	0.000	0.727	0.124
Chad 1998	-0.025 **	-0.001 **	0.000	0.108 **	0.011	0.000	0.535	0.036
Benin 1996	-0.058 **	-0.006 **	0.000	0.095 **	0.031 **	0.000	0.858	0.074
Morocco 1992	-0.051 *	0.001	0.047	0.017	0.012	0.324	0.580	0.055
Cộte d'Ivoire 1994	-0.019 **	-0.001	0.022	0.050 **	-0.005	0.028	0.896	0.012
Tanzania 1991-92	-0.006 **	0.000	0.000	-0.009 *	0.001	0.091	0.792	0.024
C.A.R. 1994-95	-0.020 **	-0.001 **	0.000	0.002 **	-0.002	0.013	0.625	0.043
Madagascar 1992	-0.058 **	-0.008 **	0.000	-0.012 *	-0.022 *	0.008	0.778	0.107
Dominican Rep. 1991	-0.003 **	-0.003 *	0.000	-0.016	-0.002	0.313	0.744	0.076
Nigeria 1999	-0.066 **	-0.013 **	0.000	0.000	-0.027 *	0.046	0.767	0.472
Cameroon 1991	-0.035 **	-0.008 **	0.000	-0.042	-0.097 **	0.000	0.699	0.094
India 1992-93	-0.025 *	-0.005 **	0.000	-0.022 **	-0.006	0.004	0.920	0.282
Haiti 1994-95	-0.001 *	-0.004	0.006	-0.037	-0.016	0.421	0.700	0.176
Uganda 1995	-0.005	-0.009 **	0.000	-0.009	-0.077 **	0.000	0.431	0.081
Bangladesh 1993-94	-0.001	-0.011 **	0.001	-0.043 *	-0.009	0.095	0.925	0.382
Bangladesh 1996-97	-0.038	-0.002	0.155	-0.051	-0.002	0.252	0.904	0.412
Philippines 1993	-0.022 *	-0.003	0.001	-0.019	-0.005	0.365	0.877	0.252
India 1998-99	-0.009	-0.005 **	0.000	-0.015	-0.014 *	0.047	0.905	0.281
Zimbabwe 1994	-0.010 **	-0.003	0.000	-0.053 *	-0.034	0.010	0.456	0.150
Bolivia 1993-94	-0.045 *	-0.003 **	0.001	-0.067 *	-0.061 *	0.014	0.805	0.173

Note: Data ordered by average school participation. Selected results from a model that includes child's age, adults' education, characteristics of the head of household, and a set of cluster characteristics. *, ** indicate significance of the underlying probit coefficients at the 5, and 1 percent respectively. (a) Reports average change in probability of enrollment resulting from a change in the dummy variable from zero to one.
Source: Author's calculation from DHS data.

The right panel of Figure 3 plots simulated enrollment with a primary and a secondary school in every village against current enrollment. This specification identifies five countries where the impact of building a primary and secondary school in every cluster would be larger than 10 percentage points (Central African Republic, Madagascar, Mali, Niger, and Uganda). For some countries the impact according to this specification is quite large. For example, in Niger (1998) a primary and secondary school in every cluster would raise enrollments from about 15 to 44 percent; and in Cameroon from about 62 to 88 percent.

Figure 3: Simulated enrollment impact of placing schools in every cluster



Note: The vertical distance from the 45^{o} line shows the change in proportion enrolled associated with reducing the distance to school to zero. Labeled countries are those for which the absolute value of the change is greater than .04 for the left panel, and 0.1 for the right panel.
Source: Author's calculations from DHS data.

Despite the seemingly larger point estimates in Table 3, the large increase in school availability implied by building a primary and secondary school in every cluster would still not typically go very far toward achieving full enrollment. Even in Burkina Faso and Senegal, with some of the lowest enrollment rates, the simulation of having both schools in all clusters is associated with no increase in enrollment.

Can a reduction in distance reduce inequalities in enrollment?

The low average impacts of school availability on enrollment could be consistent with heterogeneity across children. For example a small overall impact might be the average of no impact on boys but a relatively large impact on girls. Or no impact on children from richer families but a large impact on those from poorer families. To investigate this possibility, the probit model (with distance specified as a simple linear function) is run separately for four subsets of each data set: girls from the poorest 50 percent, girls from the richest 50 percent, boys from the poorest 50 percent, and boys from the richest 50 percent. Table 4 reports the average marginal effects of the distance to primary and secondary schools, as well as the statistical significance of the underlying probit coefficients, for each of these samples.

The overall patterns are consistent with those from the aggregate model reported in Table 1. In particular, in the Western and Central African countries where overall enrollment is low, the distance to primary school tends to be statistically significant for all the subgroups. The marginal effects of distance on enrollment are very similar for boys and girls. These results are therefore not consistent with a substantial difference in the responsiveness of enrollment to the availability of schools by gender.

On the other hand, the marginal effects are quite different for poorer and richer children. For example in Mali the marginal effect of a 1 kilometer reduction in the distance to the nearest primary school is associated with a 1.3 percentage point increase in the enrollment of poorer girls but a 2.5 percentage point increase in the enrollment of richer girls. This pattern of a larger marginal effect for richer children is the same across all the Western and Central African countries where the distance to primary school is statistically significant. Nevertheless, even though the relative magnitudes are large (frequently on the order of twice as large an impact for richer than poorer children) the absolute magnitudes of these effects are small—most are on the order of a 2 or 3 percentage point increase in enrollment associated with a 1 kilometer reduction in distance.

How much do these results imply that inequality would fall if the distance to schools were substantially reduced? Figure 4 summarizes the results of simulating reductions in the distance to schools on male/female and rich/poor inequalities. For example the top left panel illustrates the simulated impact of reducing the distance to all primary schools to zero on the ratio of male to female enrollment. The horizontal axis is the actual ratio of male to female enrollment.[9] The vertical axis is the ratio of the mean predicted enrollment of males when distance to school is set to zero (with all other covariates set to their actual values) divided by the equivalent quantity for girls.[10] Points near the value of 1 are points where there is little male/female inequality. Points near the 45-degree line are points where reducing the distance to school has little impact on male/female inequality.

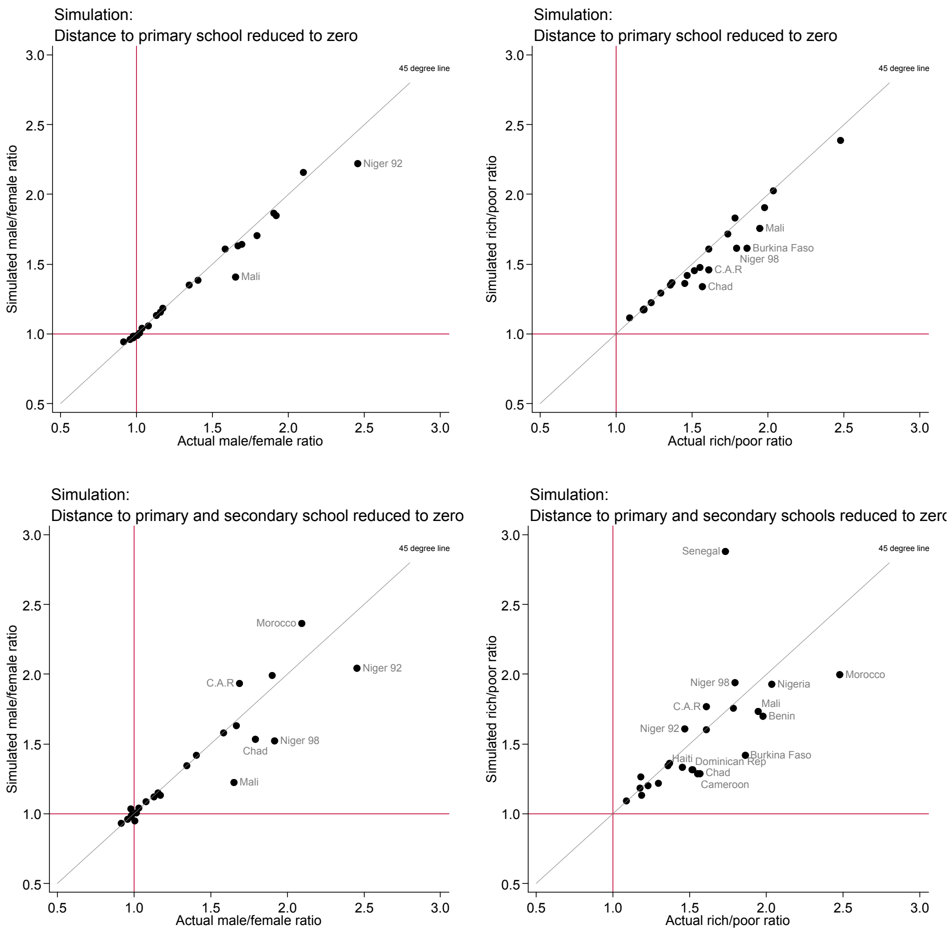
[9] It is the ratio of average predicted values, where predictions are at the actual values.
[10] The value is calculated as the weighted mean of the mean predicted values for poorer and richer children.

Table 4: Marginal effect of school distance variables in four separate probit models (by gender and household wealth), rural children aged 6 to 14

	Average marginal effects of distance								Number of observations			
	Distance to nearest primary school (km)				Distance to nearest secondary school (km)							
	Female		Male		Female		Male		Female		Male	
	Poorest 50 percent	Richest 50 percent	Poorest 50 percent	Richest 50 percent	Poorest 50 percent	Richest 50 percent	Poorest 50 percent	Richest 50 percent	Poorest 50 percent	Richest 50 percent	Poorest 50 percent	Richest 50 percent
Niger 1992	-0.006 **	-0.014 **	-0.009	-0.019 **	0.000	-0.001	0.000	0.000	1427	877	1385	915
Niger 1998	-0.025 **	-0.030 *	-0.025 *	-0.062 **	-0.001	-0.003 **	0.000	-0.002 **	1940	1241	2019	1382
Mali 1995-96	-0.013 **	-0.025 **	-0.012 **	-0.028 **	0.000	0.000	0.000	0.001	2908	1547	2825	1576
Senegal 1992-93	0.003	0.005 *	0.004 *	0.007 *	0.001	-0.003 **	0.000	-0.004 *	1361	558	1523	560
Burkina Faso 1992-93	-0.029 **	-0.027 **	-0.030 **	-0.020 **	0.000	0.000	-0.001	0.002	1660	1194	1701	1204
Chad 1998	-0.015 **	-0.020 **	-0.018 **	-0.022 **	-0.002	-0.003 *	-0.002	-0.003 **	1767	1038	1707	1085
Benin 1996	-0.020	-0.083 **	-0.018 **	-0.027	-0.001	-0.002	-0.004 *	-0.009 **	1607	874	1759	964
Morocco 1992	-0.007	0.066 **	0.002	-0.031	0.000	0.005	-0.001	0.007	1730	346	1803	360
Côte d'Ivoire 1994	0.001	-0.031 *	0.002	-0.005	-0.001	0.000	-0.001	-0.001	1750	679	1823	762
Tanzania 1991-92	-0.009	-0.014	-0.011	-0.004	0.001	-0.001	0.000	-0.001	2847	2126	2878	2048
C.A.R. 1994-95	-0.012 **	-0.008 **	-0.014 **	-0.009 **	0.002	0.001	0.001	-0.001	1240	623	1362	698
Madagascar 1992	-0.034 *	-0.040 **	-0.023 *	-0.023 *	-0.001	-0.001	0.001	0.002	1590	1143	1644	1192
Dominican Rep. 1991	-0.019	0.056 *	-0.042 *	-0.018	-0.002	0.013 *	0.000	0.010	1372	209	1540	220
Nigeria 1999	0.001	-0.010	-0.005	0.010	-0.004	-0.004	-0.003	-0.004	1764	1067	1925	1241
Cameroon 1991	-0.009	0.002	-0.012	0.001	-0.006 **	-0.007 **	-0.005 *	-0.005	848	400	843	368
India 1992-93	0.003	0.001	-0.005	-0.001	-0.001	0.000	0.000	-0.001 *	20766	14247	22713	15297
Haiti 1994-95	-0.021 **	0.012	-0.018 **	0.006	-0.001	0.001 **	-0.001	0.000	1242	457	1341	467
Uganda 1995	-0.011	-0.019 **	-0.007	-0.003	-0.003	-0.001	-0.008 **	-0.001	1741	1617	1770	1668
Bangladesh 1993-94	-0.017	0.000	-0.027	-0.022	-0.014 *	-0.011	-0.015 **	-0.011	2798	2147	2911	2138
Bangladesh 1996-97	-0.035	0.001	-0.028	-0.038 *	-0.010	0.006	-0.008	0.006	2793	2087	2845	2049
Philippines 1993	-0.002	0.020	-0.001	0.006	0.000	0.000	0.000	-0.001	3119	1222	3354	1188
India 1998-99	0.000	-0.001	0.000	0.000	-0.001	-0.001	-0.001	0.000	22636	14263	24169	15036
Zimbabwe 1994	-0.005 **	-0.013 **	-0.002	-0.008 *	-0.003 +	-0.002	-0.004 **	0.000	2385	907	2396	987
Bolivia 1993-94	-0.006		-0.006		-0.001		-0.001 **		1860		1913	

Note: Selected results from models that includes child's age, adults' education, characteristics of the head of household, and a set of cluster characteristics. *, ** indicate significance of the underlying probit coefficients at the 5, and 1 percent respectively.
Source: Author's calculation from DHS data.

Figure 4: Simulated enrollment inequality impacts of reducing the distance to schools



Note: The vertical distance from the 45° line shows the change in inequality associated with reducing the distance to school to zero. Labeled countries are those for which the absolute value of the change is greater than 0.1.
Source: Author's calculations from DHS data.

It is clear from the figure that the models imply little impact of increasing the availability of primary schools on gender inequality: almost all points lie on the 45-degree line. The only exceptions are Mali and Niger (1992) where reducing the distance to the nearest primary school to zero is associated with more than a 0.1 decline in male/female inequality.

The top right panel of Figure 4 shows the equivalent simulation for rich/poor inequalities. Again, most of the points lie on the 45-degree line, which indicates little impact of distance on inequality. For a handful of Western and Central African countries there is more than a 0.1 drop in rich/poor inequality associated with distances to primary schools equal to zero (Chad, Central African Republic, Niger 1998, Burkina Faso, and Mali).

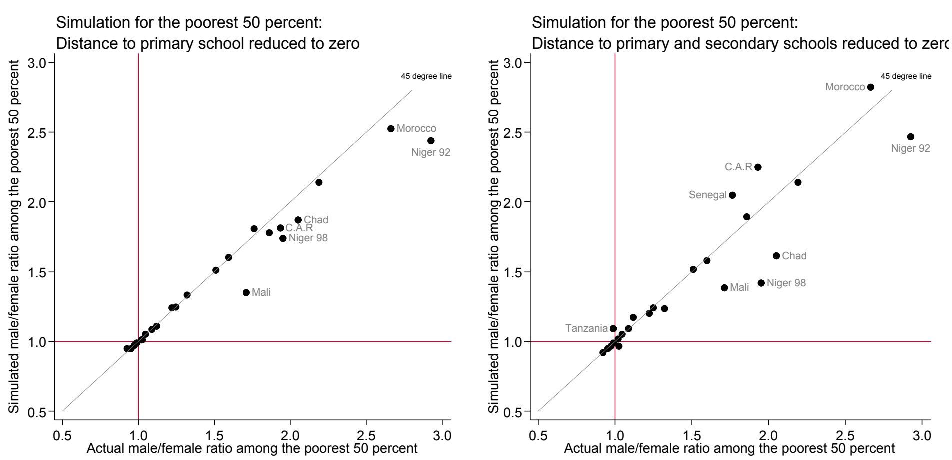
The simulation suggests that the inequality impacts of increasing the availability of both primary and secondary schools would be larger in absolute terms: substantial deviations from the

45-degree line are more common. However, whether or not this is inequality decreasing or increasing depends on the country. For example, while male/female inequality would fall by more than 0.1 in Mali, Chad, and Niger—it would increase in Central African Republic and Morocco. Similarly, rich/poor inequality would fall by more than 0.1 in several countries—but it would increase by more than 0.1 in several others.

When enrollment is low for both poorer and richer children, it is perhaps not surprising that in some cases richer children might be first to take advantage of more schools availability. Lanjouw and Ravallion (1999) discuss a model of the timing of program capture and present empirical evidence for India showing that as average enrollment of children 6 to 11 increases, it increases more for poorer children. Younger (2003) uses data from Peru and shows that as secondary school enrollment increases, it increases more among the rich than the poor. He also shows that secondary school enrollment is no more or less responsive to the distance to the nearest school for children from richer or poorer households.

These results therefore suggest that increasing the availability of schools is a limited tool to increase enrollment among girls relative to boys. Figure 5 explores the issue further by focusing on the simulated impact of distances to school set to zero on gender inequality only among the poorest 50 percent. Again most points lie close to the 45-degree line: male/female inequality in enrollment would be largely unaffected by simply placing a primary, or even a primary and a secondary, school in every cluster, even among poor households. The countries where there would be impacts on gender inequality among the poor are generally the same as those where there would be impacts on average.

Figure 5: Simulated gender enrollment inequality impacts of reducing the distance to schools among the poorest 50 percent



Note: The vertical distance from the 45^{o} line shows the change in inequality associated with reducing the distance to school to zero. Labeled countries are those for which the absolute value of the change is greater than 0.1.
Source: Author's calculations from DHS data.

6. Conclusions

Expanding the number of schools, and thereby reducing the average distance to the nearest school, is a frequently advocated approach to increasing enrollment—especially in the poorest countries where school availability is low. This systematic analysis of data from 21 developing countries suggests that while school availability is often statistically significantly associated with the probability of enrollment, the magnitude of the association is small. Interpreting the associations as the impact of availability on enrollment and simulating the effect of increase school availability suggests that increasing the availability of schools is unlikely to have much of an impact on average enrollment, and will not typically reduce inequalities by gender or wealth in enrollment. In some countries, it may even exacerbate inequalities.

These results suggest that policy interventions to raise enrollments—especially if the goal is the substantial increase required to reach universal primary enrollment—should not rely excessively on school construction. While the availability of school places might be necessary for a minimum level of schooling once a child enrolls, other interventions, such as those that raise the quality of schooling, or those that affect the demand for schooling directly through incentives to enroll or indirectly through the expected benefits to schooling, should be included in any strategy for education for all.

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